Review of "Improvements on the BRAMS wildfire-atmosphere modelling system", Menezes et al., 2025, submitted to EGUsphere.

I have several concerns with this paper – I am recommending a "soft reject" – the paper needs significant modification to be suitable for publication, but with updates and improvements may be publishable. There are important aspects to the methodology which need to be better explained, and some of the analysis seems to be flawed. My detailed comments follow.

Major issues:

Page 3, line 80; Mie theory discussion. The authors mention of "Mie theory" doesn't describe their assumptions regarding aerosol mixing state, etc., but these have been shown in past publications to result in a very large range of predicted optical depths from models. A good overview (albeit 10 years old now) can be found in Curci et al (2015) https://doi.org/10.1016/j.atmosenv.2014.09.009. Values can vary by a factor of 2 or so depending on the assumptions used, starting from the same concentration and aerosol speciation information. At that time, most aerosol radiative transfer algorithms underestimated AOD relative to observations. What is the specific approach being used in BRAMS, and how does it compare to the others in that paper? This should be decribed in the Methodology section.

Page 6, lines 157 to 160. The description in this section is inadequate for me to be able to recommend publication. Its not clear to the reader how the formulae were derived and background references are sometimes missing. Parameters are introduced but definitions and the source of data used for the parameters are not discussed. More explanation is needed here, since the formulae and some of the terms used in them are insufficient to allow the reader to understand the physical basis for the crown fire behaviour model proposed. See also my more detailed comments on manuscript pages 7 through 9, below.

Page 13, line 349, and an overall problem with Section 2.4: 0.5 degrees is about 32 km resolution, much coarser than the BRAMS grid. In order to allow a true "apples to apples" comparison of MERRA2 and BRAMS, the BRAMS output needs to be interpolated to the MERRA2 grid.

Page 17, line 466 to 475: It is concerning to me that the SFIRE model needs to be tuned for different grid cell sizes. The reason why this is necessary is not clear, since there isn't a clear description of how fire growth is handled with respect to growth across grid cells in the SFIRE grid. A description of how this is done is needed in the Methodology section. Also, page 17, line 479 refers to the need to "properly calibrate the rate of spread to avoid overpropagation" – this needs to be explained/justified. What is "overpropagation" in the authors' context, and why does it occur?

Page 19, lines 520-525 and elsewhere in the text. The authors description of the effects of non-smoke particles on the AOD versus SOD comparison seems flawed. With regards to these lines, a smoke-dominated plume would be expected to have a minor contribution relative to other sources, unless there's a similarly large event such as a dust storm deposition event happening in the vicinity of the fire.

Related issues and comments:

Page 19, line 527: "When SOD <= AOD…". Not necessarily. It could also indicate that the absorption, scattering and extinction calculated by the Mie algorithm, the aerosol optical properties, and the assumption of mixing state going into the calculation are insufficient to match the observed AOD. Note Curci's result that multicomponent aerosols' estimated AOD can vary considerably depending on the mixing state assumptions. Its equally likely that if one is looking at satellite AOD in the same location as the fire, its reasonable to expect that the AOD does indeed represent the fire as the dominant term, and hence when SOD <= AOD, the SOD calculation is underestimating the optical depth, due to some of those assumptions (not that other contributions to AOD are the cause of the differences).

Page 19, line 531. I'm hoping that the authors are not arguing that a case study where SOD << AOD in the middle of a forest fire plume is because the AOD is being affected by other sources of particulate matter. That's highly unlikely. The only way I could see this argument having validity is if the AOD doesn't change much in the satellite values upwind versus downwind of the fire. Any differences in AOD upwind versus downwind can safely be attributed to the fire emissions (the authors could do this with the AOD data; subtract the upwind background from the remaining values to get an estimate of the AOD solely due to the fire). I disagree with the statements being made in this paragraph. The authors assume that the SOD calculation itself is perfect, and doesn't take into account the variation in values that can be the result of the calculation methodology (see Curci et al., 2015).

Page 20, line 532 and Figure 3. Figure 3 also suggests that the MERRA-2 resolution is much lower than the model resolution, and hence the AOD from the satellite retrievals will be spatially averaged relative to the much higher resolution model. While the current visual comparison is useful in a general sense by showing that the plume is in fact being picked up by the satellite, it isn't quantitative, due to the resolution difference. In order to make the comparison quantitative, the authors need to use mass-conserving interpolation to map the model values to the MERRA-2 grid and compare the resulting maps at the MERRA-2 resolution. Why this is important: the upper end of the SOD scale is somewhere over 2502 (!), while the AOD scale max seems to be much lower, and more reasonable. Some of that difference may be due to the low resolution of the satellite data however - the procedure I've outlined above would allow a more quantitative comparison to be made (scatterplots of SOD versus AOD from the AOD grid values), and the associated statistics). The impression I have from the qualitative comparison is that the modelled SOD in the plume is much higher in the peak areas than the satellite values. This needs to be quantified.

Page 21, line 545, "This limitation…". I disagree on the authors' caveat here - one can subtract the upwind AOD from the rest of the values to determine the increment associated with the forest fires. From eyeballing from the images, this upwind contribution is about 0.5 or so,   So 0.5 is the background AOD upwind of the fire. The additional increment due to the fire on the AOD is about 1. The authors need to use mass-conserving interpolatate the SOD values to the AOD grid, subtract the background upwind AOD from the satellite values to generate the AOD increment associated with the fire, and then compare the resulting fields quantitatively. I think their SOD values are much higher than the AOD values - this needs to be confirmed using the above procedure.

Page 21, lines 550 to 552. I disagree with this conclusion, based on the material presented. The images as presented imply that there is a substantial overestimate of SOD from the model compared to satellite-derived AOD. The SOD values need to be converted to the AOD grid as described above, and the resulting numbers at the AOD grid cells used to show the differences. This implies that SFIRE may be greatly overestimating fire plume emissions.

Page 21, lines 556 – 558: This is why the model values need to be converted to the satellite grid in order to allow a direct comparison. I will be surprised if SOD values over 2000 are reduced to values of about 1 by that conversion, but that's what needs to be done to make this quantitative.

Page 21, line 561, "PM2.5 concentrations are extremely high". This is insufficiently quantitative. Are there no PM2.5 concentration observations of this fire that the authors can compare their results to? Failing that, what are the values of these extremely high concentrations, and how do they compare to measurements of PM2.5 during fire events found in the literature? For example, time series of PM2.5 from a large Canadian wildfire (Landis et al., 2018, https://doi.org/10.1016/j.scitotenv.2017.10.008, Figure 2) shows maximum concentrations of PM2.5 over 3000 ug/m3. Do the modelled values fall in line with other data in the literature? Later in the paper, the authors mention that the PM2.5

concentrations estimated by SFIRE reach 10^7 ug/m^3. This is unrealistically high. The authors need to provide observational evidence/references that support this number, or the reader must conclude that the SFIRE emissions estimates have very large positive biases.

Page 22, line 578 and Figure 4: The same issue as for Figure 3 occurs here. an apples to apples comparison on the satellite grid, with the satellite upwind values removed, is needed. A short description of how the satellite generates BC and OC concentration estimates in the introduction would help place this work in context.

Page 24, lines 614-616: This seems counter-intuitive: how does a lower resolution imply higher values of SSA?

Page 25, lines 620 to 625, "Taken together…" I disagree. The authors images indicate that SFIRE in this implementation has large positive biases in SOD compared to AOD, and that unrealistically high PM2.5 concentrations are being generated, consistent with the SOD values. The additional analysis I've described above would help confirm this.

Page 27, lines 635-642: These simulated values are MUCH higher than any observations of PM even in the immediate fire environment of which I'm aware. I think that this and the SOD values suggest that the model has unacceptably high positive biases in emissions. The authors need to present evidence from the literature that PM levels can reach these values - I think this is confirming that the model has very high positive biases. "below 1000 ug/m3" is being described as a relatively low concentration, and 10^7 ug/m3 doesn't seem realistic to me.

Page 27, lines 644-645. The paper does not describe how the model generates and uses a plume injection height for the emissions. This needs to be added to the methodology, since it could have a substantial impact on the model results. How is the height of the plume calculated, and how is the emitted mass distributed in the vertical in the BRAMS coordinate system? The high positive biases could result, for example, from the emissions being treated as a surface flux (I'm trying to figure out how they could get numbers that high at 2km resolution). This needs to be clarified in the methodology.

Page 30, lines 710 and Figure 8 analysis. I think that the authors may have the sign wrong in their interpretation of Figure 8. The Figure has been labelled "No Fire – Fire" so a positive value indicates that the quantity is larger in the absence of smoke. That is, the smoke is *decreasing* the longwave radiation, not *increasing* it as this section suggests. That is, the presence of smoke results in surface cooling, something which has been seen in other papers (e.g. Makar et al,, 2021, https://acp.copernicus.org/articles/21/10557/2021/, Figure

20., Makar et al, 2015 https://doi.org/10.1016/j.atmosenv.2014.12.003) i.e. what is being shown is a local cooling effect, not a local heating effect. I think the authors need to redo this section based on the sign of the differences.

Minor issues:

Page 1, paper title. Title needs to incorporate "a case study", e.g. "...modelling system: a case study based on the Serta wildfire". The work does not constitute a broader evaluation of the model for multiple fires (the current title might give the reader that impression).

Page 2, line 45: The authors need to be more specific on what they mean when they quote dynamic feedbacks here. Note that there are dynamic feedback effects in the form of the aerosol direct and indirect effects on radiative transfer which can have a substantial impact on PBL heights, meteorology and consequently on forest fire emissions and plume height. I think the authors mean aerosol direct radiative effects. Might want to compare to Makar et al, 2019 https://acp.copernicus.org/articles/21/10557/2021/ where some similar results have been shown.

Page 2, line 45: there are a few papers missing here on models with a similar intent to BRAMS (wildfire smoke forecasting). Some examples: HRRR-Smoke (cf. Chow et al., BAMS, 2022: https://doi.org/10.1175/BAMS-D-20-0329.1, Chen et al., GMD, (2019 https://gmd.copernicus.org/articles/12/3283/2019/), Anderson et al., GMD, 2024, https://gmd.copernicus.org/articles/17/7713/2024/). The latter two models also simulate fire spread (through comparison between historical hotspots and rates of spread by ecosystem) and associated fuel burned is used to calculate heat release, in turn used to calculate the plume rise height. These models also estimate the fuel burned from crown, smoldering and residual phases of the fires. The approach taken in these models should be compared and contrasted with the work of the authors of the current submission: what are the differences that make the authors' work unique/better/an improvement relative to these models? For example, these models use a statistical approach for determining fire spread (historical data is used to determine the average area burned on a per hotspot per forest classification type - if the authors are incorporating a more explicit fire spread algorithm into their work, that would be an advancement relative to these papers. Anderson et al (2024) also provides estimates of emissions resulting from several different approaches - a similar attempt should be made here to place the authors work in the context of the forest fire emissions algorithms currently in use elsewhere in the world.

Page3. Line 54. I couldn’t find anything in the paper explicitly explaining how fire ignition is handled for applications of SFIRE. I think its using satellite FBP to locate the fire ignition points and then calculates its own FBP values thereafter – but I'm not sure. Please clarify this in the text.

Page 3, line 80. Note that there has been historically a large range of estimates of these parameters with different assumptions such as the aerosol mixing state. A good overview (albeit 10 years old now) can be found in Curci et al (2015) https://doi.org/10.1016/j.atmosenv.2014.09.009. Values can vary by a factor of 2 or so depending on the assumptions used, starting from the same concentration and aerosol speciation information. At that time, most aerosol radiative transfer algorithms underestimated AOD relative to observations. What is the specific approach being used in BRAMS, and how does it compare to the others in that paper?

Page 3, line 83, "high-resolution". Please state both the BRAMS and SFIRE horizontal grid cell size used in the study, at this point in the text.

Page 4, line 86: please replace "validation" with "evaluation" throughout the paper. Validation implies a model is "valid" once the process is complete, "evaluation" describes the model's current performance with respect to observations.

Page 4, Methodology section. This section would be greatly improved with a figure showing the SFIRE grid and the BRAMS grid in the vicinity of the fire used as a case study. Also, the resolution of the elevation data should be mentioned here. Later in the paper, it appears that SFIRE is operating on a 200m grid, but the elevation data has a 1km grid, and BRAMS was on a 2km grid. While there is some discussion later on the relative impact of resolution, it is not clear why lower-than-SFIRE resolution topography was used in the current SFIRE application, given the impact of slope on SFIRE results. This choice needs to be justified.

Page 5, line 119, "traditional methods". Please be specific with regards to what is meant by "traditional methods" here, including (a) reference(s). Presumably the traditional methods had a 50% underestimate?

Page 5, line 130, "with a radius". How is this radius predicted *a priori*? What information is used to determine this initial high-resolution SFIRE domain?

Page 5, line 132, "In subsequent steps". Its unclear how BRAMS lets SFIRE know it has to do a fire calculation in a given grid cell. I'm guessing that an FRP value from a satellite is used? This needs to be clarified.

Page 5, line 134-135. The resolution of BRAMS when SFIRE is being used needs to be mentioned here. The energy transfer on a per grid-cell area basis will be a lot less with a 2km resolution model than a 200 m resolution model. I'm surprised that there's no discussion of the influence of resolution on the coupling between BRAMS and SFIRE. There is also no discussion (a few lines of text would be helpful) describing how SFIRE determines the height of the plume. Presumably this is determined within SFIRE, but this has not been made clear, and the methodology used has not been discussed. I'm assuming that the SFIRE energy has not been passed to BRAMS with the expectation that BRAMS' meteorology will be able to resolve the rise due to the plume – BRAMS grid cell size (2km, mentioned much later in the paper) is not sufficient to resolve a forest fire plume.

Page 6, Figure 1. Rather than subroutine names (which tell the reader nothing about what's being done in the subroutine), please modify this diagram to have a brief process description phrase (and the subroutine name, if present at all, in a bracket thereafter). What the subroutines *do* is of more interest to the reader than the subroutine name, and would better help the reader to understand the sequence of events in the model.

Page 7, lines 162 to 165. There needs to be more background information on "fuel behaviour model 10" and the sources of the data (references) used within it. It is not clear for example whether the formulae which follow in this section are part of this model, or are new formula introduced by the authors.

Page 7, line 164: "Physical and chemical properties of the fuel" – are these part of this fuel model or introduced here (and how were they derived in either case)?

Page 7, line 165: spelling mistake, "crow" should be "crown".

Page 7 , line 166 and throughout the subsequent lines and formula: I assume Rinicialization should be Rinitialization?

Page 7, line 167: Iinitialization: How was this derived (from what data/reference)? Units of Iinitialization? The reader at this point has the impression that some of these terms are coming from NFFL 10, but not what the terms are or what they represent. The paper needs bracket's after each term, eg. "Iinitiatlization (insert descriptive definition here)". What is the definition of the critical minimum fireline intensity, for example? Note that readers of the paper may be completely unfamiliar with the NFFL models. What do the parameters represent, and how were they and these formulae derived?

Page 7, line 176: what is the definition and units of reaction intensity?

Page 7, line 176, NFFL fuel behaviour model 10 – given that this seems to be a critical addition to the existing SFIRE model, there needs to be some description of what this model is, how its parameters were derived, and references for it. Why was this model chosen instead of some other model? Is it appropriate in some way for the forest type for the case study used here? Given that other models are available, why use this one?

Page 7, line 177, surface-area-to-volume ratio: Surface to volume ratio of *what?* The forest canopy? The trees in the canopy, etc? The terms in the equations need to be better defined and explained.

Page 7, line 183. Why this particular number (40%)? Is that part of NFFL or something the authors are setting? Please justify cases like this where numerical limits have been introduced.

Page 7, line 190: R10 has been introduced without explanation. Or should this be R6.1 (why 6.1 m instead of 10?)?

Page 8, line 204: "It is used to estimate the degree of crowning". Suggest "Here, we make use of the empirical function as defined by Van Wagner (1989)". Also, please define what is meant by the degree of crowning.

Page 8, line 215: Byram fireline intensity is known to specialists in forest fire combustion, but not to most readers of the paper. Please define it.

Page 8, equation (9) and other equations: Its not clear which formula are part of NFFL 10 and which have been introduced by the authors. Please clarify this.

Page 8, equation (10): several new terms have been introduced here without definition.

Page 9, line 235: What numbers are used for Wsurface, Hlow, what do they depend on (or are they constants), what are the references for them?

Page 9, additional comment on section 2.2.1. Some discussion on how the rate of spread values are used within SFIRE, in terms of SFIRE's high resolution grid is needed. Is there an internal timestep applied to work out the fuel burned from the rate of spread, and does it vary from one SFIRE grid cell to the next? Or is a single rate of spread used? How is the direction of the spread determined? I'm wondering how SFIRE deals with things like changes in topography, etc., and how the R values are used to determine burned area A, from the standpoint of a high resolution grid. I've seen many approaches to this in the literature, ranging from explicit wind vectors at every grid cell and forward trajectories being used to determine spread, to ellipsoidal spread being assumed – what's being used here? A sketch explanatory figure showing an example SFIRE grid within a BRAMS cell, and some description of how the fire spread is calculated from one BRAMS timestep to the next (and whether a smaller timestep is used for SFIRE calculations) is needed.

Page 9, line 251. Why these particular values? For example, do they best represent the forest types for the case study? Some justification is needed.

Page 9, comment on section 2.3: no mention is made of how SFIRE calculates plume height and distributes emissions in the vertical. This is crucial to getting the correct concentrations of forest fire smoke, and needs to be included in the methodology section. Similarly, how SFIRE identifies ignition locations has not been mentioned (and should be part of the description).

Page 10, line 283: flaming and smoldering phases mentioned, but not the residual phase. Why has residual phase been left out?

Page 11, line 288: authors state that the setup provides accurate simulation of wildfire emission and their atmospheric impacts prior to quantitatively demonstrating this. Statements such as this should be in the discussion or conclusions, after the case has been made.

Page 11, line 292-293. The subsequent SOD vs AOD analysis does not touch on the extent to which aerosol mixing state can influence predicted SOD values. Aerosol optical properties are a function of the aerosol mixing state, the aerosol size distribution, and the aerosol composition (Curci et al., 2015). How are these variables represented in the version of BRAMS used here? For example, has a specific composition been assumed for the aerosols, a specific size distribution, and a specific aerosol mixing state? This information needs to be provided. The authors have mentioned that the test version used here does not include chemistry - what about the aerosol microphysics effects (nucleation, condensation, coagulation)?

Page 11, line 295, characterization of aerosol microphysical properties: Details? What properties were determined and what was the conclusion?

Page 11, lines 304-306: This is the first mention of the domain used for the simulations - I assume that the model was being run for Portugal, then? This should have been made clear right at the start of the paper at the end of the Introduction or in the Methodology sections.

Page 11, line 311: The previous discussion referred to one of these models, leaving the reader with the impression that that specific model is the one being tested here. Were all 13 used, then? Please clarify (and explain why model 10 was preferred).

Page 13, line 338, 1km terrain data. Its not clear whether this data is being used by SFIRE, BRAMS or both (and note that the resolution of BRAMS and SFIRE has yet to be mentioned).

Page 13, lines 358 to 360: Should be mentioned earlier, in the model setup discussion. How does this assumption (a single particle size) compare to actual size distributions from real fires, and how might that be expected to influence optical depths? These potential issues shoulld be acknowledged. cf. for Radke et al., 1988, 1990:

Radke, L. F., Hegg D. A., Lyons J. H., Brock C. A., Hobbs P. V, Weiss R. , and Rassmussen R., Airborne measurements on smokes from biomass burning In Aerosols and climate, ed. P. V. Hobbs and M. P. McCormick, 411–22. Hampton, VA: A. Deepak, 1988.

Radke, L. F., J. H. Lyons, P. V. Hobbs, D. A. Hegg, D. V. Sandberg, and D. E. Ward, Airborne monitoring and smoke characterization of prescribed fires on forest lands in western Washington and Oregon: Final report. Gen. Tech. Rep. PNW-GTR-251. Portland: U.S.D.A. Forest Service, Pacific Northwest Research Station, 1990.

Examples from the second of these papers for smoke particle distributions are given below, for the authors' information:

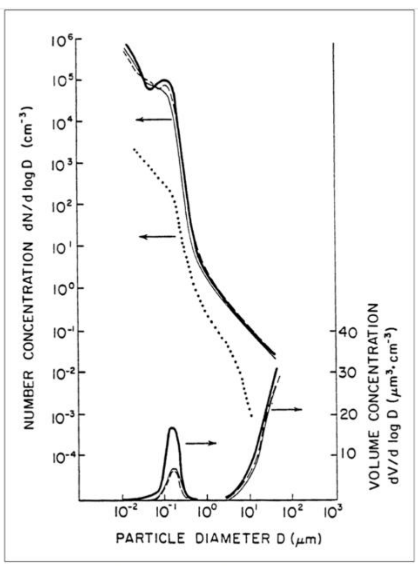

Figure 20—Number concentrations versus size of particles measured near the center of the plume and 3.3 km downwind of the burn on July 23, 1982, at 1636 PDT (heavy solid line), 1741 PDT(dashed line) and 1759 PDT (solid line). The ambient particle size distribution is also shown (dotted line).

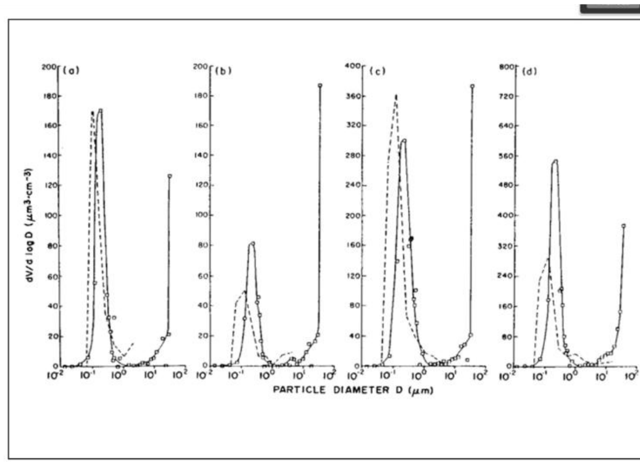

Figure 29—Volume concentration versus size of particles measured with the aerosol sizing system near the center of the plume from the burns: (a) 0954 PDT, July 25, 1982; (b) 1605 PDT, September 23, 1982; (c) 1718 PDT, September 23, 1982; and (d) 1700 PDT, September 23, 1982. The dashed lines show results derived from the cascade microbalance impactor (mass/density of particles).

Page 14, lines 376 to 377, "consistent with wildfire aerosol characteristics"? Wildfire aerosols have a size distribution, and are not monodisperse. What has been done here needs to be acknowledged as a simplification, and its potential impacts need to be discussed. The distributions of Radke et al 1990 (see above Figures) do have a volume distribution peak at about 0.2 um diameter or so, but there is also a second (and sometimes much higher) peak at about 50 to 60 um diameter). What the authors have here is a reasonable proxy for the peak of the PM2.5 mass distribution, but not the overall particle mass. So "consistent with wildfire aerosol characteristics" isn't quite justified. Try "consistent with the location of the peak in the fine mode portion of emitted smoke mass".

Page 14, lines 396 – 397 and 402: please justify the choice of the complex refractive index values used. The authors should compare the complex refractive index values employed to the single-component values for Black Carbon, Primary Organic Carbon and Secondary Organic Carbon quoted in Curci et al 2015, Table 2. One thing that is really striking is that the authors' 700nm refractive index' imaginary component has a much lower value than in the references used there (the authors : 0.04. Curci's references: 0.44 to 0.71, and I've seen similar high values elsewhere for black carbon refractive index' imaginary component). I think the authors' value will underestimate the backscattering associated with black carbon. Real components look ok. Suggest they do a sensitivity test with a higher imaginary component to check on the impact. Alternatively, if the refractive index values are based on observations from actual smoke particles, then the latter reference might be useful to indicate that the organic fraction dominates the radiative effects. Note that the impact of black carbon may be higher if a core-shell approach to Mie theory has been used.

Page 15, line 411. I was having difficulty following this sentence - does MERRA-2 have a product that they refer to as the BC and OC components at 500nm? There needs to be a brief description of what the MERRA2 products are, and how they are derived (i.e. how does the satellite retrieval attribute AOD to BC versus OC?).

Page 15, line 413. Ditto. How does MERRA-2 infer mass of BC and OC (short few sentence summary needed). My point here is that the satellite doesn't measure these things directly, and the retrieval processing must be making assumptions regarding aerosol mixing state, etc., to attribute portions of the retrieval to BC vs OC. Can the authors provide some reassurance to the reader that what MERRA2 is measuring and what the model is generating are sufficiently similar to be worth comparing (again, a short description would help)?

Page 15, equation (19): Equation 19 needs some justification/explanation. The sensible heat flux is usually thought of as an infrared flux, while 550nm is in the visible. Or should H_{sens} be H_{sens}(lambda) in this equation?

Page 16, lines 429 to 434. So the feedback effects mentioned earlier are via the aerosol direct effect then. This should be mentioned earlier, towards the end of the Introduction or in the Methodology sections.

Page 16, line 442, "particularly the relative abundance of BC and OC". But this depends on the authors' assumed relative distribution of BC and OC in the complex refractive index values, does it not? That is, the authors' complex refractive indexes are much more like OC than typical BC values – they have a priori assumed a low BC value in their choice of indexes. Which may be justified if those were based on smoke particle observations. They seem to be assuming very small values of the magnitude of the complex index, hence relatively small values of BC compared to OC.

Page 17, line 456-458. This is the first time that the resolution has been mentioned. It should have been mentioned in the model description. Its also not clear here whether this refers to the resolution of SFIRE within WRF or BRAMS, or the resolution of WRF or BRAMS (I suspect the former, but the resolution of the latter has not been mentioned, and the relative resolution of WRF and BRAMS could also influence model results). There needs to be a description of the SFIRE and BRAMS grid cell size earlier in the paper under Methodology.

Page 17, line 458, “This can amplify…” I thought the authors mentioned earlier that the resolution of the topography data was 1km, not the 200m of SFIRE as applied here. If anything, the BRAMS-SFIRE grid should have more *gentle* slopes than the 20m grid in WRF-SFIRE in that case. Its not clear to me how a lower resolution model can have a greater sensitivity to terrain-induced effects; the higher resolution should have greater local slopes. The explanation needs more work: how does topographic assimilation result in a stronger gradient field?   If the authors were to convert Figure 2 to show the derivative of topography with respect to horizontal dimensions, they'd get steeper slopes in the WRF side than the BRAMS side. But the text here seems to suggest the coarser resolution is somehow more prone to slope-influenced growth. Please clarify this.

Page 17, line 467, “ignition misalignment”. This is the first mention of ignition in the paper, and how ignition is handled in SFIRE is needed in the Methodology section. I'm assuming that, because this is a historical case study, the ignition time and spatial location is known? If I've understood the paper correctly, satellite-observed FBP is used to determine ignition locations and times? Please clarify explicitly how the locations for ignition are determined in the model. The authors should also explain what they mean by "ignition misalignment" in this context.

Page 17, lines 468-469.   This is the first mention in the paper of how SFIRE calculates spread between adjacent SFIRE grid cells. A diagram of how this is done on the SFIRE grid in the Methodology is needed.

Page 17, line 470. Not sure what’s meant by “in a timely manner” here.

Page 18, lines 476-477. Given that SFIRE is operating within BRAMS as a parameterization, its not clear to me why couldn't a higher resolution topography database be used with the SFIRE part of BRAMS-SFIRE? Would that not reduce the sensitivity issue?

Page 18, line 486. I assume this should be "consume biomass fuel" not "consume biomass fuel models"? Models aren't being consumed.

Page 29, lines 672-673. This begs the question of whether or not BRAMS-SFIRE includes a module for convective transport (or is the assumption that any vertical transport is from the resolved winds at 2km resolution)? Again, I'm wondering how the model handles smoke plume-rise. Related, lines 689-690: Is the transport of pollutants due to convection included in SFIRE or BRAMS? This gets back to my question regarding plume rise methodology. Also - lines 689 to 705 might be better placed in the Introduction; background information.

Page 30, line 720, page 33, line 751: Don't need to include all co-author names in references in the main text, use "et al".

Page 33, line 760, "BC-rich smoke" But is BC-rich smoke what has been simulated? See earlier comments regarding complex refractive index values. Also note that earlier text emphasized the relative importance of OC compared to BC.

Page 37, Figure 10: the authors should include a figure with the profiles of the air temperature differences resulting from the fire, to show the cooling/heating impact.